*Chapter 1*

# Introduction

*Planck made two great discoveries in his lifetime: the energy quantum and Einstein* [Miller 81]

## 1.1 Einstein's Impact on Twentieth Century Physics

When one mentions the word 'relativity' the name Albert Einstein springs to mind. So it is quite natural to ask what was Einstein's contribution to the theory of relativity, in particular, and to twentieth century physics, in general. Biographers and historians of science run great lengths to rewrite history.

Undoubtedly, Abraham Pais's [82] book, *Subtle is the Lord*, is the definitive biography of Einstein; it attempts to go beneath the surface and gives mathematical details of his achievements. A case of mention, which will serve only for illustration, is the photoelectric effect.

Pais tells us that Einstein proposed $E_{\text{max}} = h\nu - P$, where $\nu$ is the frequency of the incident (monochromatic) radiation and $P$ is the work function — the energy needed for an electron to escape the surface. He pointed out that [this equation] explains Lenard's observation of the light intensity independence of the electron energy. Pais, then goes on to say that first

> $E$ [sic $E_{\text{max}}$] should vary linearly with $\nu$. Second, the slope of the $(E, \nu)$ plot is a universal constant, independent of the nature of the irradiated material. Third, the value of the slope was predicted to be Planck's constant determined from the radiation law. None of this was known then.

This gives the impression that Einstein singlehandedly discovered the photoelectric law. This is certainly inaccurate. Just listen to what J. J. Thomson [28] had to say on the subject:

> It was at first uncertain whether the energy or the velocity was a linear function of the frequency... Hughes, and Richardson and Compton were however able to

> show that the former law was correct... The relation between maximum energy and the frequency can be written in the form $\frac{1}{2}mv^2 = k\nu - V_0 e$, where $V_0$ is a potential characteristic of the substance. *Einstein suggested that k was equal to h, Planck's constant*. [italics added]

Pais asks "What about the variation of the photoelectron energy with light frequency? One increases with the other; nothing more was known in 1905." So it is not true that "At the time Einstein proposed his heuristic principle, no one knew how $E$ depended on $\nu$ beyond the fact that one increases with the other." ... And this was the reason for Einstein's Nobel Prize.

### 1.1.1 *The author(s) of relativity*

Referring to the second edition of Edmund Whittaker's book, *History of the Theory of Relativity*, Pais writes

> Forty years latter, a revised edition of this book came out. At that time Whittaker also published a second volume dealing with the period from 1910 to 1926. His treatment of the special theory of relativity in the latter volume shows how well the author's lack of physical insight matches his ignorance of the literature. I would have refrained from commenting on his treatment of special relativity were it not for the fact that his book has raised questions in many minds about the priorities in the discovery of this theory. Whittaker's opinion on this point is best conveyed by the title of his chapter on this subject: 'The Relativity Theory of Poincaré and Lorentz.'

Whittaker ignited the priority debate by saying

> In the autumn of the same year, in the same volume of the *Annalen der Physik* as his paper on Brownian motion, Einstein published a paper which set forth the relativity theory of Poincaré and Lorentz with some amplifications, and which attracted much attention. He asserted as a fundamental principle the *constancy of the speed of light*, i.e. that the velocity of light *in vacuo* is the same for all systems of reference which are moving relatively to each other: the assertion which at the time was widely accepted, but has been severely criticized by later writers. In this paper Einstein gave the modifications which must now be introduced into the formulae for aberration and the Doppler effect.

Except for the 'severe criticism,' which we shall address in Sec. 4.2.1, Whittaker's appraisal is balanced. Pais's criticism that "as late as 1909 Poincaré did not know that the contraction of rods is a consequence of the two Einstein postulates," and that "Poincaré therefore did not understand one of the most basic traits of special relativity" is an attempt to discredit Poincaré in favor of Einstein. In fact, there have been conscientious attempts at demonstrating Poincaré's ignorance of special relativity.

The stalwarts of Einstein, Gerald Holton [88] and Arthur Miller [81] have been joined by John Norton [04] and Michel Janssen [02]. There has been a growing support of Poincaré, by the French, Jules Leveugle [94], Christian Marchal, and Anatoly Logunov [01], a member of the Russian Academy of Sciences. It is, however, of general consensus that Poincaré arrived at the two postulates first — by at least ten years — but that "he did not fully appreciate the status of both postulates" [Goldberg 67]. Appreciation is fully in the mind of the beholder.

There is a similar debate about who 'discovered' general relativity, was it Einstein or David Hilbert? These debates make sense if the theories are correct, unique and compelling — and most of all the results they bear. In this book we will argue that they are not unique. It is also very dangerous when historians of science enter the fray, for they have no means of judging the correctness of the theories. However, since it makes interesting reading we will indulge and present the pros and cons of each camp.

Why then all the appeal for Einstein's special theory of relativity? Probably because the two predictions of the theory were found to have practical applications to everyday life. The slowing down of clocks as a result of motion should also apply to all other physical, chemical and biological phenomena. The apparently inescapable conclusions that a twin who goes on a space trip at a speed near that of light returns to earth to find his twin has aged more than he has, and the decrease in frequency of an atomic oscillator on a moving body with the increase in mass on the moving body which is converted into radiation, all have resulted in paradoxes.

All this means that the physics of the problems have as yet to be understood. Just listen to the words of the eminent physicist Victor Weisskopf [60]:

> We all believe that, according to special relativity, an object in motion appears to be contracted in the direction of motion by a factor $[1-(v/c)^2]^{1/2}$. A passenger in a fast space ship, looking out the window, so it seemed to us, would see spherical objects contracted into ellipsoids.

Commenting on James Terrell's paper on the "Invisibility of the Lorentz contraction" in 1960, Weisskopf concludes:

> ... is most remarkable that these simple and important facts of the relativistic appearance of objects have not been noticed for 55 years.

It is well to recognize that what appears as to be a firmly established phenomenon keeps popping up in different guises. It is the same type of remarks that the space contraction is a 'psychological' state of mind, and not a 'real' physical effect, that prompted Einstein to reply:

> The question of whether the Lorentz contraction is real or not is misleading. It is not 'real' insofar as it does not exist for an observer moving with the object.

Here, Einstein definitely committed himself to the 'reality' of the Lorentz contraction.

#### 1.1.1.1 *Einstein's retraction of these two postulates and the existence of the aether*

The cornerstones of relativity are the equivalence of all inertial frames, and the speed of light is a constant in all directions *in vacuo*. These postulates were also those of Poincaré who uttered them at least seven years prior to Einstein. So what makes Einstein's postulates superior to those of Poincaré?

Stanley Goldberg [67] and Arthur Miller [73] tell us that Poincaré's [04] statements

> the laws of physical phenomena must be the same for a stationary observer as for an observer carried along in a uniform motion of translation; so that we have not and cannot have any means of discerning whether or not we are carried along in such a motion,

and

> no velocity can surpass that of light,

were elevated to "a priori postulates" [Goldberg 67] which "stood at the head of his theory." These postulates also carry the name of Einstein. Why then would Einstein ever think of retracting them?

If time dilatation and space contraction due to motion are actual processes then there is no symmetry between observers in different inertial frames. The first postulate of relativity is therefore violated [Essen 71]. Einstein used *gedanken* experiments which is an oxymoron. Consider what Einstein [16] has to say about a pair of local observers on a rotating disc:

> By a familiar result of the special theory of relativity the clock at the circumference — judged by $K$ — goes more slowly than the other because the former is in motion and the latter is at rest. An observer at the common origin of coordinates capable of observing the clock at the circumference by means of light would therefore see it lagging behind the clock beside him. As he will not make up his mind to let the velocity of light along the path in question depend explicitly on the time, he will

> interpret his observations as showing that the clock at the circumference 'really' goes more slowly than the clock at the origin.

First the uniformly rotating disc is not an inertial system so the special theory does not apply. Second, *local* observers cannot discern any changes to their clocks or rulers as to where they are on the disc because they shrink or expand with them. It is only to us Euclideans that these variations are perceptible.

If the velocity of light is independent of the velocity of its source, how then can the outward journey of a light signal to an observer moving at velocity $v$ be $c + v$, on its return it travels with a velocity $c - v$? Although this violates the second postulate, such assertions appear in the expression for the elapsed time of sending out a light signal from one point to another and back again in the Michelson–Morley experiment whose null result they hope to explain. They also appear alongside Einstein's relativistic velocity composition law in his famous 1905 paper "On the Electrodynamics of Moving Bodies."

Also in that paper is his 'definition' of the velocity of light as the ratio of "light path" to the "time interval." But we are not allowed to measure the path of the light ray and determine the time it took, for $c$ has been elevated to a universal constant! "How can two units of measurement be made constant by definition?" Essen queries.

In his first attempt to explain the bending of rays in a gravitational field, Einstein [11] claims

> For measuring time at a place which, relative to the origin of the coordinates, has a gravitation potential $\Phi$, we must employ a clock which — when removed to the origin of coordinates — goes $(1 + \Phi/c^2)$ times more slowly than the clock used for measuring time at the origin of coordinates. If we call the velocity of light at the origin of coordinates $c_0$, then the velocity of light $c$ at a place with the gravitational potential $\Phi$ will be given by the relation

$$c = c_0 \left(1 + \frac{\Phi}{c^2}\right).$$

> *The principle of the constancy of the velocity of light holds good according to this theory in a different form from that which usually underlies the ordinary theory of light.* [italics added]

On the contrary, this violates the second postulate which makes no reference to inertial nor non-inertial frames. And is his equation a cubic equation for determining $c$?

It did not take Max Abraham [12] long to point this out stating that Einstein had given "the death blow to relativity," by retracting the invariance of *c*. Abraham said he warned "repeatedly against the siren song of this theory. . . [and] that its originator has now convinced himself of its untenability." What Abraham objected most to was that even if relativity could be salvaged, at least in part, it could never provide a "complete world picture," because it excludes, by its very nature, gravity.

Einstein also uses the same Doppler expression for the frequency shift. The Doppler shift is caused by the motion of the source with respect to the observer. "There is, therefore, no logical reason why it should be caused by the gravitational potential, which is assumed to be equivalent to the acceleration times distance" [Essen 71]. Thus Einstein is proposing another mechanism for the shift of spectral lines that employs accelerative motion rather than the relative motion of source and receiver. Does the acceleration of a locomotive cause a shift in the frequency of its whistle? or is it due to its velocity with respect to an observer on a stationary platform? But no, Einstein has replaced the product of acceleration and distance with the gravitational potential — which is *static*! Just where a clock is in a gravitational field will change its frequency. This is neither a shift caused by velocity nor acceleration.

Everyone would agree that Einstein removed the aether. Whereas Hertz considered the aether to be dragged along with the motion of a body, Lorentz considered the aether to be immobile, a reference frame for an observer truly at rest. On the occasion of a visit to Leyden in 1920, Einstein [22a] had this to say about the aether:

> . . . the whole change in the conception of the aether which the special theory of relativity brought about, consisted in taking away from the aether its last mechanical quality, namely, its immobility. . . . according to the general theory of relativity space is endowed with physical qualities; in this sense, therefore, there exists an aether. . . . space without aether is unthinkable; for in such a space there not only would be no propagation of light, but also no possibility of the existence for standards of space and time (measuring rods and clocks), nor therefore any space time intervals in the physical sense. But this aether may not be thought of as endowed with the quality characteristic of ponderable media, as consisting of parts which may be tracked through time. The idea of motion may not be applied to it.

Essentially what Einstein is saying that what was not good for special relativity is good for general relativity for "We know that [the new aether] determines the metrical relations in the space-time continuum." How is it needed for the propagation of light signals and yet has not the characteristics of a medium? Einstein's real problem is with rotations for "Newton might no less well have called his absolute space 'aether;' what is essential is merely that besides observable objects, another thing, which is not perceptible, must be looked upon as real, to enable acceleration or rotation to be looked upon as something real."

This is five years after Einstein's formulation of general relativity, and his desire is to unite the gravitational and electromagnetic fields into "one unified conformation" that would enable "the contrast between aether and matter [to] fade away, and, through the general theory of relativity, the whole of physics would become a complete system of thought." The search for that utopia was to occupy Einstein for the remainder of his life.

#### 1.1.1.2 *Which mass?*

In Lorentz's theory two masses result depending on how Newton's law is expressed, i.e.

$$F = \frac{d}{dt}(mv),$$

or

$$F = ma,$$

where $a$ is the acceleration. Both forms of the force law coincide when the mass is independent of the velocity, but not so when it is a function of the velocity. If the force is perpendicular to the velocity there results the transverse mass,

$$m_t = \frac{m_0}{\sqrt{(1-\beta^2)}},$$

while if parallel to the velocity there results the longitudinal mass,

$$m_l = \frac{m_0}{(1-\beta^2)^{3/2}}.$$

While it is true that a larger force is required to produce an acceleration in the direction of the motion than when it is perpendicular to the motion, it "is unfortunate that the concept of two masses was ever developed, for the [second] form of Newton's law is now recognized as the correct one" [Stranathan 42].

In the early days of relativity the relativistic mass was written $m = \frac{4}{3}E/c^2$, and not $m = E/c^2$. Einstein was aloof to the factor of $\frac{4}{3}$ — which was a consequence of the Lorentz transform on energy — but not to there being two masses. According to Einstein [05] "with a different definition of the force and acceleration we would obtain different numerical values for the masses; this shows that we must proceed with great caution when comparing different theories of the motion of the electron." Apart from 'numerical' differences, Kaufmann's experiments identified the mass as the transverse mass, but this did not prevent Einstein [06a] to propose an experimental method to determine the ratio of the transverse to the longitudinal mass.

According to Einstein the ratio of the transverse to longitudinal mass would be given by the ratio of the electric force, $eE$, to the potential, $V$, "at which the shadow-forming rays get deflected," i.e.

$$\frac{m_t}{m_l} = \frac{\rho}{2}\frac{E_x}{V},$$

where $\rho$ is the radius of curvature of the shadow-forming rays and $E_x$ is the electric field in the $x$-direction. As the 'definition' of the longitudinal mass, $m_l$, Einstein takes

$$\text{kinetic energy} = \frac{1}{2}m_l v^2.$$

It would be very difficult for Einstein to get this energy as a nonrelativistic approximation of a relativistic expression for the kinetic energy.

Einstein's contention that

> A change of trajectory evidently is produced by a proportional change of the field only at electron velocities at which the ratio of the transverse to longitudinal mass is noticeably different from unity

is at odds with his assumption of the validity of the equation of motion,

$$m_0\frac{d^2x}{dt^2} = -eE_x,$$

which holds "if the square of the velocity of the electrons is very small compared to the square of the velocity of light." The mass of the electron $m_0$ is not specified as to whether it is the transverse or longitudinal mass, or a combination of the two.

This example shows that Einstein was not attached to his relativity theory as he is made out to be. Why is it that the same types of contradictions and incertitudes found in Poincaré's statements are used as proof as to his limitations as a physicist, while there is never mention of them in Einstein's case?

### 1.1.1.3 *Conspiracy theories*

In order to defend the supremacy of German science, David Hilbert, with the help of Hermann Minkowski and Emil Wiechert, set out to deny Poincaré the authorship of relativity. Hilbert was the last in a long line of illustrious Göttingen mathematicians who sought to retain the dominance of the University which boasted of the likes of Carl Friedrich Gauss, Bernhard Riemann and Felix Klein. Whereas there existed a friendly competition between Felix Klein and Poincaré [Stillwell 89], Hilbert's predecessor, there was jealousy between Hilbert and Poincaré, which was only exasperated when Poincaré won the Bolyai prize in mathematics for the year 1905. Ironic as it may be, János Bolyai was the co-inventor of hyperbolic geometry, and the rivalry between Klein and Poincaré had to do with the development of that geometry.

As the story goes, Arnold Sommerfeld [04], an ex-assistant of Klein's, Gustav Herglotz and Wiechert were working on superluminal electrons during the fall of 1904 through the spring of 1905. In the summer months of 1905, beginning on the notorious date of the 5th of June, the Göttingen mathematicians organized seminars on the 'theory of electrons,' in which there was a session on superluminal electrons chaired by Wiechert on the 24th of July.

The date of the 5th of June coincided with Poincaré's [05] presentation of his paper, "Sur la dynamique de l'électron," to the French Academy of Sciences. The printed paper was published and sent out to all correspondents of the Academy that Friday, the 9th of June. The earliest it could have arrived in Göttingen was Saturday the 10th, or given postal delays it would have arrived no latter than the following Tuesday, the 13th of June.[a] In that

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[a]These dates are reasonable since the other German physics bi-monthly journal, *Fortschrift der Physik* had a synopsis of the Poincaré paper in its 30th of June issue. Given the publication delay, it would make the 10th of June arrival date of the *Comptes Rendus* issue more likely.

paper Poincaré supposedly declared that no material body can go faster than the velocity of light in vacuum, and this threw a wrench into the works of the Göttingen school [Marchal].

However, this is nothing different than what Poincaré [98] had been saying since 1898 when he postulated the invariance of light *in vacuo* to all observers, whether they are stationary or in motion. Or, to what Poincaré reiterated in 1904: "from these results, *if they are confirmed* would arise a new mechanics [in which] no velocity could surpass that of light." So the all-important date of the publication date of 5th of June to the proponents of the conspiracy theory [Leveugle 04] is a red herring for it said only what he had said before on the limiting velocity of light. Moreover, there was a continual boycott of Poincaré's relativity work in such prestigious German journals as *Annalen der Physik*. Consequently, there was no contingency for the appearance of Einstein's paper when it did. But let us continue.

So the plot was hatched that some German, of minor importance and one who was willing to take the risks of plagiarism, had to be found that would reproduce Poincaré's results without his name. Now Minkowski knew of Einstein since he had been his student at the ETH[b] from 1896–1900. Einstein was also in contact with Planck, since Einstein's summary of the work appearing in other journals for the *Beiblätter zu der Annalen der Physik* earned him a small income. In fact, there is one review of Einstein of a paper by A. Ponsot "Heat in the displacement of the equilibrium of a capillary system," that appeared in the *Comptes Rendu* **140** just 325 pages before Poincaré's June 5th paper. To make matters worse, an article by Weiss, which appeared in the same issue of *Comptes Rendu*, was summarized in the November issue of the Supplement, but not for Poincaré's paper.

Neither that paper nor its longer extension that was published in the *Rendiconti del Circolo Matematico di Palermo* [06] were ever summarized in the *Beiblätter*. Surely, these papers would have caught the eye of Planck, who was running the *Annalen*, and was known to be in correspondence with Einstein not only in this connection, but, also with regard to questions on quanta. Einstein had also published some papers on the foundations

[b]The Eidgenössische Technische Hochschule (ETH) was then known as the Eidgenössische Polytechnikum; the name was officially changed in 1911.

of thermodynamics during the years 1902–1903 in the *Annalen* whose similarity with those of J. Willard Gibbs was "quite amazing" even to Max Born [51].

Thus, the relativity paper was supposedly prepared by the Göttingen mathematicians and signed by Einstein who submitted it for publication at the end of June, arriving at the offices of the *Annalen* on the 30th of June. Einstein was an outsider, being considered a thermodynamicist, with a lot to gain and little, if nothing, to lose. The paper fails to mention either Lorentz or Poincaré, and, for that matter, contains no references at all. If there was a referee for the paper,[c] other than Planck himself, it would have been obvious that the transformation of the electrodynamic quantities went under the name of Lorentz, with Lorentz's parameter $k(v)$ replaced by Einstein's $\varphi(v)$, both ultimately set equal to 1, and the relativistic addition law had already been written down by Poincaré as a consequence of the Lorentz transform in his 1905 paper on "Sur la dynamique de l'électron." Although Einstein derives the relativistic composition law in the same way as Poincaré, he provides a new generalization when the composition of Lorentz transformations are in different planes, for that also involves rotations. It has been claimed that there was no connection between Lorentz and Einstein for Einstein gets the wrong expression for the transverse mass in his "Electrodynamics of moving bodies," while Lorentz errs when he subjects the electric current to a Lorentz transformation [Ohanian 08]. But, it is clear from his method of derivation from the Lorentz force, that Einstein's error was a typo. Einstein's paper appeared in the 26th of September issue of the *Annalen*, and Planck lost no time in organizing a symposium on his paper that November, which, in the words of von Laue, was "unforgettable."

Not all is conjecture, certain things are known. First, Poincaré worked in friendly competition with Klein in studying universal coverings of surfaces. What initiated Poincaré on his studies of hyperbolic geometry was

[c]Apparently the paper was handled by Wilhelm Röntgen, a member of the *Kuratorium* of the *Annalen*, who gave it to his young Russian assistant, Abraham Joffe [Aufray 99]. Joffe noted that the author was known to the *Annalen*, and recommended publication. That an experimental physicist should have handled the paper, and not the only theoretician on the *Kuratorium* — Planck — would have made such a referring procedure extremely dubious.

an 1882 letter of Klein to Poincaré who informed him of previous work by Schwarz. Second, it was Klein who brought Hilbert to Göttingen. When criticized about his choice, Klein responded "I want the most difficult of all." Third, Klein was known to pass on important letters and scientific material to Hilbert. Fourth, since Klein and Poincaré were on good terms and in contact, it would be unthinkable that Klein did not know of Poincaré's work on relativity, and that Klein would have passed this on to Hilbert. Fifth, there was a lack of "kindred spirit" [Gray 07] between Poincaré and Hilbert from their first meeting in Paris in 1885. Sixth, Poincaré was "unusually open about his sources," [Gray 07] and non-polemical, while Hilbert had a tremendous will who thought every problem was solvable. Lastly, Poincaré's work on relativity was actively boycotted in Germany, and later in France thanks to Paul Langevin. Thus, it is unthinkable that Hilbert was in the dark about relativity theory prior to 1905. His colleague, Minkowski, became interested in electrodynamics through reading Lorentz's papers. According to C. Reid, in "Hilbert," Hilbert conducted a joint seminar with Minkowski. A year after their study, in 1905, they decided to dedicate the seminar to a topic in physics: the electrodynamics of moving bodies. Hilbert was often quoted as saying "physics is too important to be left to the physicists." What is truly unbelievable that the discover of relativity and two models of hyperbolic geometry would not even once think there was a relation between the two. Everything else is conjecture, even Einstein's supposed receipt of the latest issue of Volume CXL of *Comptes Rendus*, vested as a reviewer for the *Beiblätter*, on Monday the 12th of June in the Berne Patent Office. Undoubtedly, that would have created a dire urgency to finish his article on the electrodynamics of a moving body [Auffray 99]. But wherever the real truth may lie, there cannot be any doubt that Planck played a decisive role in Einstein's rise to fame.

The behavior of Langevin to a fellow countryman is even more baffling when we realize that he was the first French physicist to learn of the "new mechanics" of Poincaré, which would later be known as relativity, but without the name of its author. Langevin had accompanied Poincaré to the Saint-Louis Congress of 1904 where he presented his principle of relativity. It is hardly admissible that Langevin was not familiar of all Poincaré's publications especially when Poincaré [06] dedicated a whole section of his 1906 article in the *Rendiconti* to him, entitling it "Langevin Waves," and stating

> Langevin has put forth a particularly elegant formulation of the formulas which define the electromagnetic field produced by the motion of a single electron.

Yet, in his obituary column of Poincaré, Langevin fails to note Poincaré's priority over Einstein's writing

> Einstein has rendered the things clearer by underlining the new notions of space and time which correspond to a group totally different than the conserved transformations of rational mechanics, and asserting the generality of the principle of relativity and admitting that no experimental procedure could ascertain the translational movement of a system by measurements made on its interior. He has succeeded in giving definitive form to the Lorentz group and has indicated the relations that exist between the same quantity simultaneously made on each of two systems in relative movement.
>
> Henri Poincaré arrived at the same equations in the same time following a different route, his attention being directed to the imperfect form which the formulas for the transformation had been given by Lorentz. Familiar with the theory of groups, he was preoccupied to find the invariants of the transformation, elements which are unaltered and thanks to which it is possible to pronounce all the laws of physics in a form independent of the reference system; he sought the form that these laws must have in order to satisfy the principle of relativity.

This could not have appeared in a more appropriate place: *Revue de Métaphysique et de Morales*!

Another priority feud also erupted between Einstein and Hilbert over general relativity in November 1915. It ended with the publication of papers with the unpretentious titles of "The foundation of the general theory of relativity," by Einstein, and "The foundations of physics," by Hilbert. Historians of science make Einstein's theory the ultimate theory of gravitation with titles like "How Einstein found his field equations," [Norton 84], and "Lost in the tensors: Einstein's struggle with covariance principles" [Earman & Glymour 78]. In the opinion of O'Rahilly [38], "Einstein's theory, which delights every aesthetically minded mathematician, is a much less grandiose affair as judged and assessed by the physicist." He points out that Walther Ritz arrived at prediction of a perihelion advance of the planets in 1908. We will use his same force equation to show he could have obtained the other predictions of general relativity in Sec. 3.8.2. Furthermore, the same experimental tests of these equations can be obtained with far more simplicity, as we shall see in Chapter 7. The proponents of the conspiracy theory claim that Einstein's conciliatory letter of December to Hilbert may be due, in part, for the favor that Hilbert did for him ten years earlier.

The defenders of Einstein belittle Poincaré for his "lack of insight into certain aspects of the physics involved" [Goldberg 67]. The same can be said of Einstein; in a much quoted letter to Carl Seelig on the occasion of the 50th 'anniversary' of relativity, Einstein writes:

> The new feature was the realization of the fact that the bearing of the Lorentz-transformations transcended their connection with Maxwell's equations and was concerned with the nature of space and time in general. A further result was that the Lorentz invariance is a general condition for any physical theory. This was for me of particular importance because I had already previously found that Maxwell's theory did not account for the micro-structure of radiation and could therefore have no general validity.

In a letter to von Laue in 1952, Einstein elaborated what he meant by a "second type" of radiation pressure:

> one has to assume that there exists a second type of radiation pressure, not derivable from Maxwell's theory, corresponding to the assumption that radiation energy consists of indivisible point-like localized quanta of energy $h\nu$ (and of momentum $h\nu/c$, $c =$ velocity of light), which are reflected undivided. The way of looking at the problem showed in a drastic and direct way that a type of immediate reality has to be ascribed to Planck's quanta, that radiation, must, therefore, possess a kind of molecular structure as far as energy is concerned, which of course contradicts Maxwell's theory.

Maxwell's equations together with the Lorentz force satisfy the Lorentz transform so it is difficult to see that the transformation is more general than what it transforms. In addition, the discovery of Planck's radiation law did not contradict the Stefan–Boltzmann radiation law, nor provide a new type of radiation. Here, Einstein is confusing macroscopic laws with the underlying microscopic processes that are entirely compatible with those laws when the former are averaged over all frequencies of radiation. Consequently, there is no second type of radiation pressure.

What the conspiracy theories have in common with their opponents is the presumption that the end result is correct. What authority did Poincaré's June paper of 1905 have for dashing the efforts of Sommerfeld's investigations on superluminal electrons? Weber was no stranger to superluminal particles nor was Heaviside. In all the years preceding that paper, there was no authority bearing down upon them even though the mathematical structure of relativity had been set in place. What was supposedly new about Einstein's paper was the liberation of space and time from an electromagnetic framework, as he claimed in his letter to Seelig. But is this true?

#### 1.1.1.4 *Space-time in Einstein's world*

The conventional way of rebuffing the conspiracy theories is "to show the nature of Poincaré's ideas and approach that prevented him from producing what Einstein achieved" [Cerf 06]. Einstein was not so unread as he would have us believe for he used Poincaré's method — radar signaling — in discussing simultaneous events, and falls into the same trap as Poincaré did.

Poincaré asks us to consider two observers, $A$ and $B$, who are equipped with clocks that can be synchronized with the aid of light signals. $B$ sends a signal to $A$ marking down the time instant in which it is sent. $A$, on the other hand, resets his clock to that instant in time when he receives the signal. Poincaré realized that such a synchronization would introduce an error because it takes a time $t$ for light to travel between $B$ and $A$. That is, $A$'s clock would be behind $B$'s clock by a time $t = d/c$, where $d$ is the distance between $B$ and $A$. This error, according to Poincaré is easy to correct: Let $A$ send a light signal to $B$. Since light travels at the same speed in both directions, $B$'s clock will be behind $A$'s by the same time $t$. Therefore, in order to synchronize their clocks it is necessary for $A$ and $B$ to take the arithmetic mean of the times arrived at in this way. This is also Einstein's result.

Certainly the definition of the velocity $v = d/t$ seems innocuous enough. But, as Louis Essen [71] has pointed out it is possible to define the units of any two of these terms. Normally, one measures distance in meters and time in seconds so the velocity is meters per second. But making the velocity of light constant "in all directions and to all observers whether stationary or in relative motion" is tantamount to making $c$ a unit of measurement, or what will turn out to be an *absolute* constant. According to Essen, "the definition of the unit of length or of time must be abandoned; or, to meet Einstein's two conditions, it is convenient to abandon both units."

The two conditions that Essen is referring to is the dilatation of time and the contraction of length. There is no new physical theory, but, "simply a new system of units in which $c$ is constant" so that either time or length or both must be a function of $c$ such that their ratio, $d/t$, gives $c$. This is not what Louis de Broglie [51] had to say:

> Poincaré did not take the decisive step. He left to Einstein the glory of having perceived all the consequences of the principle of relativity and, in particular, of having clarified through a deeply searching critique of the measures of length and duration, the physical nature of the connection established between space and time by the principle of relativity.

So by elevating the velocity of light to a universal constant, Einstein implied that the geometry of relativity was no longer Euclidean. The number $c$ is an absolute constant for hyperbolic geometry that depends for its value on the choice of the unit of measurement. To the local observers there is no such thing as time dilatation nor length contraction. These distortions are due to our Euclidean perspective. It is all a question of 'frame of reference.'

Poincaré after having written down his relativistic law of the composition of velocities should have realized that the only function which could satisfy such a law is the hyperbolic tangent, which is the straight line segment in Lobachevsky (velocity) space. Thus, time and space have no separate meaning, but only their ratio does.

Consider Einstein's two postulates which he enunciated in 1905:

(i) The same laws of electrodynamics and optics will be valid for all frames of reference for which the equations of mechanics hold.
(ii) Light is always propagated in empty space with a definite velocity $c$, which is independent of the state of motion of the emitting body.

Match them against Poincaré's first two postulates as he pronounced them in 1904:

(i) The laws of physical phenomena should be the same whether for an observer fixed, or for an observer carried along in a uniform movement of translation; so that we could not have any means of discerning whether or not we are carried along in such a motion;
(ii) Light has a constant velocity and in particular that its velocity is the same in all directions.

Now Poincaré introduces a third postulate, which Pais makes the following comment:

> The new mechanics, Poincaré said, is based on three hypotheses. The first of these is that bodies cannot attain velocities larger than the velocity of light. The second is (I use modern language) that the laws of physics shall be the same in all inertial frames. So far so good. Then Poincaré introduces a third hypothesis. 'One needs to make still a third hypothesis, much more surprising, much more difficult to accept, one which is of much hindrance to what we are currently used to. A body in translational motion suffers a deformation in the direction in which it is displaced. . . However strange it may appear to us, one must admit that the third hypothesis is perfectly verified.' It is evident that as late as 1909 Poincaré did not know that the contractions of rods is a consequence of the two Einstein postulates. Poincaré therefore did not understand one of the most basic traits of special relativity.

Whether or not rods contract or rotate when in motion will be discussed in Sec. 9.9, but it appears that Pais is reading much too much into what Poincaré said as to what he actually did. In Sec. 4 of "Sur la dynamique de l'électron" published in 1905, entitled "The Lorentz transformation and the principle of least action," Poincaré shows that both time dilatation and space contraction follow directly from the Lorentz transformations. By the Lorentz transformation,

$$\delta x' = \gamma l(\delta x - \beta ct), \quad \delta y' = l\delta y, \quad \delta z' = l\delta z, \quad \delta t' = \gamma l(\delta t - \beta\delta x/c),$$

it follows that for measurements made on a body at the same moment, $\delta t = 0$, in an inertial system moving with a relative velocity $\beta = v/c$ along the $x$-axis, the body undergoes contraction by a factor $\gamma^{-1}$ when viewed in the unprimed frame when we set $l = 1$. It is therefore very strange that Poincaré would reintroduce this as a third hypothesis when it is a consequence of Lorentz's transformation which he accepts unreservedly. As Poincaré was prone to writing popular articles and books he may have thought that the contraction of rods were sure to catch the imagination of the layman.

The problem is in the interpretation of what is meant by the second postulate regarding the constancy of light, which is usually interpreted as the velocity of light relative to an observer, whether he be stationary or moving at a velocity $v$. Thus, instead of obtaining values $c + v$ or $c - v$ for the velocity of light, for an observer moving at $\pm v$ relative to the source, one would always 'measure' $c$. A frequency would therefore not undergo a Doppler shift, contrary to what occurs.

According to Einstein's prescription, the time taken for a light signal to complete a 'back-and-forth' journey over a distance $d$ is the arithmetic average of the two

$$t = \frac{1}{2}d\left[\frac{1}{c+v} + \frac{1}{c-v}\right] = d\frac{c}{c^2 - v^2}.$$

We are thus forced to conclude that instead of obtaining the velocity $c$, we get the velocity $c(1 - v^2/c^2)$, which differs from the former in the presence of a second order term, $-v^2/c^2$. Rather, if we use the relativistic velocities $(c + v)/(1 + v/c)$ and $(c - v)/(1 - v/c)$, we obtain

$$t = \frac{1}{2}d\left[\frac{1+v/c}{c+v} + \frac{1-v/c}{c-v}\right] = d/c,$$

and the second-order effect *disappears*, just as it would in the Michelson–Morley experiment [cf. Sec. 3.2].

It is not as Einstein claims: "The quotient [distance by time] is, in agreement with experience, a universal constant $c$, the velocity of light in empty space." The 'experience' is the transmission of signals back and forth, like those envisioned by Poincaré. In this setting, the 'principle' of the constancy of light is untenable [Ives 51].

The velocities of light in the out and back directions $c_o$ and $c_b$ will, in general, be different. If the distance traversed by the light signal is $d$, the total time for the outward and backward journey is, according to Einstein,

$$t = \frac{1}{2}\left(\frac{d}{c_o} + \frac{d}{c_b}\right) = \left(\frac{c_o + c_d}{c_o c_d}\right)\frac{d}{2}. \tag{1.1.1}$$

But, according to the principle of relativity, there should be no difference in the velocities of light in the outward and backward directions, so that this principle decrees

$$t = \frac{d}{c}. \tag{1.1.2}$$

Equating (1.1.1) and (1.1.2) yields [Ives 51]

$$\frac{(c_o + c_b)/2}{c_o c_b} = \frac{1}{c},$$

which can easily be rearranged to read:

$$\frac{c}{\sqrt{(c_o c_b)}} = \frac{2\sqrt{(c_o c_b)}}{(c_o + c_b)} \leq 1. \tag{1.1.3}$$

The inequality in (1.1.3) follows from the arithmetic-geometric mean inequality which becomes an equality only when $c_o = c_b = c$. Thus, if there are no superluminal velocities, the latter case must hold, for if not, one of the two velocities, $c_o$ or $c_b$ must be greater than $c$.

A similar situation occurs for the inhomogeneous dispersion equation of a wave [cf. Sec. 11.5.6],

$$\omega^2 = c^2\kappa^2 + \omega_0^2,$$

where $\omega$ and $\kappa$ are the frequency and wave number, and $\omega_0$ is the critical frequency below which the wave becomes attenuated. Differentiation of the dispersion equation gives

$$\omega d\omega = c^2\kappa \, d\kappa.$$

Introducing the definitions of phase and group velocities, $u = \omega/\kappa$ and $w = d\omega/d\kappa$, it becomes apparent that $u > c$ implies $w < c$ [Brillouin 60]. Since $uw = c^2$, the equivalence of the two velocities requires the critical frequency to vanish and so restores the isotropy of space.

Einstein [05] uses absolute velocities to show that two observers traveling at velocities $\pm v$ would not find that their clocks are synchronous while those at rest would declare them so. He considers light emitted at $A$ at time $t_A$ to be reflected at $B$ at time $t_B$ which arrives back at $A$ at time $t'_A$. If $d$ is the distance between $A$ and $B$, the time for the outward and return journeys are

$$t_B - t_A = \frac{d}{c+v},$$

and

$$t'_A - t_B = \frac{d}{c-v},$$

respectively. Since these are not the same, Einstein concludes that what seems simultaneous from a position at rest is not true when in relative motion. But, in order to do so, Einstein is using absolute velocities: the velocity on the outward journey is $c + v$, and the velocity of the return journey is, $c - v$, and so violates his second postulate. If the relativistic law of the composition of velocities is used, instead, the total times for outward and return journeys become the same, which is what is found to within the limits of experimental error [Essen 71].

Einstein then attempts to associate physical phenomena with the fact that clocks in motion run slower than their stationary counterparts, and rods contract when in motion in comparison with identical rods at rest. He considers what is tantamount to the Lorentz transformations, as a rotation through an imaginary angle, $\theta$,

$$x' = x \cosh\theta - ct \sinh\theta, \quad ct' = ct\cosh\theta - x\sinh\theta,$$

at the origin of the system in motion so that $x' = 0$. He thus obtains

$$x/t = c\tanh\theta, \quad t' = t\surd(1 - v^2/c^2) = t/\cosh\theta. \tag{1.1.4}$$

He then concludes that clocks transported to a point will run slower by an amount $\frac{1}{2}tv^2/c^2$ with respect to stationary clocks at that point, which is valid up to second-order terms.

Rather, what Einstein should have noticed is that

$$\theta = \tanh^{-1} v/c = \frac{1}{2}\ln\left(\frac{1+v/c}{1-v/c}\right)$$

is the relative distance in a hyperbolic velocity space whose 'radius of curvature' is $c$. Space and time have lost their separate identities, and only appear in the ratio $v = x/t$ whose hyperbolic measure is $\theta = \bar{v}/c$. The role of $c$ is that of an *absolute* constant, whose numerical value will depend on the arbitrary choice of a unit segment. By raising the velocity of light to a universal constant, Einstein implied that the space is no longer Euclidean. Euclidean geometry needs standards of length and time; in this sense Euclidean geometry is *relative*. In terms of meters and seconds, the speed of light is $3 \times 10^8$ m/s. If there was no Bureau of Standards we would have no way of defining what a meter or second is.

Not so in Lobachevskian geometry where angles determine the sides of the triangle. In Lobachevskian geometry lengths are absolute as well as angles. The 'radius of curvature' $c$ is no longer an upper limit to the velocities, but, rather, defines the unit of measurement. Lobachevskian geometries with different values of $c$ will not be congruent. As $c$ approaches infinity, Lobachevskian formulas go over into their Euclidean counterparts.

The exponential distance,

$$e^{\bar{v}/c} = \left(\frac{1+v/c}{1-v/c}\right)^{1/2} = \frac{\nu'}{\nu}, \tag{1.1.5}$$

is the ordinary longitudinal Doppler factor for a shift in the frequency, $\nu'$, due to a moving source at velocity $v$. In the Euclidean limit, $\theta \approx x/ct$ and (1.1.5) reduces to the usual Doppler formula [Varičak 10]:

$$\nu' = \nu(1 + v/c).$$

It is undoubtedly for this reason that both Einstein and Planck found non-Euclidean geometries distasteful. For as Planck remarked [98]

> It need scarcely be emphasized that this new conception of the idea of time makes the most serious demands upon the capacity of abstraction and projective power of the physicist. It surpasses in boldness everything previously suggested in speculative natural phenomena and even in the philosophical theories of knowledge: non-Euclidean geometry is child's play in comparison. And, moreover, the principle of relativity, unlike non-Euclidean geometry, which only comes seriously into consideration in pure mathematics, undoubtedly possesses a real physical significance. The revolution introduced by this principle into the physical conceptions of the world is only to be compared in extent and depth with that brought about by the introduction of the Copernican system of the universe.

Prescinding Planck's degrading remarks concerning non-Euclidean geometries, we can safely conclude that

> the distortion effects due to the spatial contraction and time dilatation of moving objects can be perceived by an observer using a Euclidean metric and clock. To local observers in hyperbolic space, there is no possible way of discerning these distortions because their rulers and clocks shrink or expand with them. All the 'peculiar consequences' are based on the issue of 'frame of reference.'

What is truly tragic is that Poincaré never realized that his models of non-Euclidean geometries were pertinent to relativity. According to Arthur Miller [73]

> For a scientist of Poincaré's talents the awareness of Lorentz's theory should have been the impetus for the discovery of relativity. Poincaré seemed to have all the requisite concepts for a relativity theory: a discussion of the various null experiments to first and second order accuracy in $v/c$; a discussion of the role of the speed of light in length measurements; the correct relativistic transformation equations for the electromagnetic field and the charge density; a relativistically invariant action

> principle; *the correct relativistic equation for the addition of velocities*; the concept of the Lorentz group; a rudimentary of the four-vector formalism and of four-dimensional space; a correct relativistic kinematics... [italics added]

so what went wrong? Miller claims that "his relativity was to be an inductive one with the laws of electromagnetism as the basis of all of physics." This, according to Miller, prevented him from grasping the "universal applicability of the principle of relativity and therefore the importance of the constancy of the velocity of light in all inertial frames." In other words, the equations are right but the deductions are wrong. One can deduce what he likes from the equations as long as it is compatible with experiment.

While Miller [81] acknowledges that both Poincaré and Einstein, "simultaneously and independently," derived the relativistic addition law for velocities, "only Einstein's view could achieve its full potential." He further claims that Poincaré never proved "the independence of the velocity of light from its source. . .." These assertions have no justification at all: Poincaré did not have to prove anything, the velocity addition law negates ballistic theories. It is also not true that "Lorentz's theory contained special hypotheses for this purpose." No special hypotheses are needed since the velocity addition law is a direct outcome of the Lorentz transformations. Here is a clear intent to disparage Poincaré.

And where is the experimental verification of Einstein's theory as opposed to Poincaré's? Or, maybe, Poincaré just did not go far enough? According to Scribner [64] the whole of the kinematical part of Einstein's 1905 paper could have been rewritten in terms of aether theory. So according to him, the aether would play the role of the caloric in Carnot's theory which, by careful use, did not invalidate his results. Carnot never 'closed' his cycle for that would have meant equating the heat absorbed at the hot reservoir with the heat rejected at the cold reservoir since, according to caloric theory, heat had to be conserved.

Where Einstein puts into quotation marks "stationary" as opposed to "moving" it does not imply a physical difference because one is relative to the other. Moreover, the distinctions between "real" and "apparent" must likewise be abandoned. If there is no distinction between the two, then why should Einstein have taken exception to Varičak's remark that Einstein's "contraction is, so to speak, only a psychological and not a physical fact." This brought an immediate reaction from Einstein to the effect that Varičak's note "must not remain unanswered because of the confusion that it could bring about." After all these years has the confusion been abated?

To condemn Lorentz and Poincaré for their belief in the aether is absurd. The aether for them was the caloric for Carnot. But did the caloric invalidate Carnot's principle? And if Carnot has his principle, why does Poincaré not have his? Carnot's principle still stands when the scaffolding of caloric theory falls.

Another analogy associates Poincaré to Weber, and Einstein to Maxwell. Weber needed charges as the seat of electrical force, while Maxwell needed the aether as the medium in which his waves propagate. Maxwell's circuital equations make no reference to charges as the carriers of electricity.

Miller [73] asserts that Poincaré did not realize "in a universal relativity theory the basic role is played by the energy and momentum instead of the force." But it was Lorentz's force that was able to bridge Maxwell's macroscopic field equations with the microscopic world of charges and currents.

It is clear that Poincaré did not want to enter into polemics with Einstein. And Einstein, on his part, admits that his work was preceded by Poincaré. After a critical remark made by Planck on Einstein's first derivation of $\Delta m = \Delta E/c^2$, to the effect that it is valid to first-order only, the following year Einstein [06b] makes another attempt. In this study he proposes to show that this condition is both necessary and sufficient for the law of momentum, which maintains invariant the center of gravity, citing Poincaré's 1900 paper in the Lorentz *Festschrift*. He then goes on to say

> Although the elementary formal considerations to justify this assertion are already contained essentially in a paper of Poincaré, I have felt, for reasons of clarity, not to avail myself of that paper.

Even though Einstein clearly admits to Poincaré's priority no one seems to have taken notice of it.

On July the 5th 1909, Mittag-Leffler, editor of *Acta Mathematica* writes to Poincaré to solicit a paper on relativity writing

> You know without doubt Minkowski's *Space and Time* published after his death, and also the ideas of Einstein and Lorentz on the same problem. Now, Fredholm tells me that you have reached the similar ideas before these other authors in which you

> express yourself in a less philosophical, but more mathematical, manner. Would you write me a paper on this subject . . . in a comprehensible language that even the simple geometer would understand.

Poincaré never responded.

Then there was the letter of recommendation of Poincaré's to Weiss at the ETH where he considers Einstein

> as one of the most original minds that I have met. I don't dare to say that his predictions will be confirmed by experiment, insofar as it will one day be possible.

Notwithstanding, Einstein writes in November 1911 that "Poincaré was in general simply antagonistic." Relativity was probably just a word to him, since it was he who postulated the 'principle of relativity.' But it is true that Poincaré looked to experimental confirmation for his principle. Be that as it may, what is truly incomprehensible is Poincaré's lack of appreciation of the velocity addition law, for that should have put him on the track of introducing hyperbolic geometry. Then the distortions in space and time could be explained as the distortion we Euclideans observe when looking into another world governed by the axioms of hyperbolic geometry. To the end of his life, Poincaré maintained that Euclidean geometry is the stage where nature enacts her play, never once occurring to him that his mathematical investigations would have some role in that enactment.

Now Poincaré was more than familiar with Lorentz's contraction of electrons when they are in motion. He even added the additional, non-electromagnetic, energy necessary to keep the charge on the surface of the electron from flying off in all directions. The contraction of bodies is likened to the inhabitants of this strange world becoming smaller and smaller as they approach the boundary. The absolute constant needed for such a geometry would be the speed of light which would determine the radius of curvature of this world. In retrospect, it is unbelievable how Poincaré could have missed all this.

It is also said that Poincaré was using the principle of relativity as a fact of nature, to be disproved if there is one experiment that can invalidate it. This is not much different than the second law of thermodynamics. In fact when Kaufmann's measurements of the specific charge initially tended to favor the Abraham model of the electron [cf. Sec. 5.4.1], Poincaré [54] appears to have lost faith in his principle for

> [Kaufmann's] experiments have given grounds to the Abraham theory. The principle of relativity may well not have been the rigorous value which has been attributed to it.

Kaufmann's experiments were set-up to discriminate between various models proposed for the dependency of the mass of the electron on its speed. And if the Lorentz model had been found wanting, Einstein had much more to lose since his generalization of Lorentz's electron theory to all of matter would certainly have been its death knell. Einstein had this to say in his *Jahrbuch* [07] article:

> It should also be mentioned that Abraham's and Bucherer's theories of the motion of the electron yield curves that are significantly closer to the observed curve than the curve obtained from the theory of relativity. However, the probability that their theories are correct is rather small, in my opinion, because their basic assumptions concerning the dimensions of the moving electron are not suggested by theoretical systems that encompass larger complexes of phenomena.

The last sentence is opaque, for what do the dimensions of a moving electron share with larger complexes of phenomena? And how are both related with Kaufmann's deflection measurements? Einstein may not have liked Abraham's model, but Abraham did because, according to him, it was based on common sense. It must be remembered that Lorentz's theory of the electron was also a model. According to Born and von Laue, Abraham will be remembered for his unflinching belief in "the absolute aether, his field equations, his *rigid* electron just as a youth loves his first flame, whose memory no later experience can extinguish."

But how rigid could Abraham's electron be if the electrostatic energy depended on its contraction when in motion? That is everyone will agree that "Abraham took his electron to be a rigid spherical shell that maintained its spherical shape once set in motion... [yet] a sphere in the unprimed coordinate system becomes, in the primed system, an ellipsoid of revolution" [Cushing 81]. The unprimed system is related to the prime system by a dilation factor, equal to the inverse FitzGerald–Lorentz contraction, which elongates one of the axes into the major axis of the prolate ellipsoid. In the Lorentz model, one of the axes is shortened by the contraction factor so that an oblate ellipsoid results. In fact, as we shall see in Sec. 5.4.4, that the models of Abraham and Lorentz are two sides of the same coin, which are related in the same way that hyperbolic geometry is related to elliptic geometry, or a prolate ellipsoid to an oblate ellipsoid.

If we take Einstein's [Northrop 59] remark:

> If you want to find out anything about theoretical physicists, about the methods they use, I advise you to stick closely to one principle: don't listen to their words, fix your attention on their deeds.

at face value, then according to Einstein's own admission, there is no difference between the Poincaré–Lorentz theory and his. Whether the mass comes from a specific model of an electron in motion, or from general principles which makes no use of the fact that the particle is charged or not, they merge into the exact same formula for the dependence of mass on speed.

### 1.1.2 *Models of the electron*

At the beginning of the twentieth century several models of the electron were proposed that were subsequently put to the test by Kaufmann's experiments involving the deflection of fast moving electrons by electric and magnetic fields. The two prime contenders were the Abraham and Lorentz models. If mass of the electron were of purely electromagnetic origin, it should fly apart because the negative charges on the surface would repel one another. There is a consensus of opinion that it was for this reason Abraham chose a rigid model of an electron which would not see the accumulation of charge that a deformed sphere would.

Miller [81] contends that Abraham "chose a rigid electron because a deformable one would explode, owing to the enormous repulsive forces between its constituent elements of charge." Even a spherical electron would prove unstable without some other type of binding forces. In that case, "the electromagnetic foundations would be excluded from the outset," according to Abraham. In order to calculate the electrostatic energy Abraham needed an expression for the capacitance for an ellipsoid of revolution. This he found in an 1897 paper by Searle. The last thing he had to do was to postulate a dependence of the semimajor axis of revolution upon the relative velocity $\beta = v/c$. 'Rigid' though the electron may be, Abraham evaluated the electrostatic energy in the primed system where a sphere of radius $a$ turns into a cigar-shaped prolate ellipsoid with semimajor axis $a/\sqrt{(1-\beta^2)}$. So Abraham's rigid electron was not so rigid as he might have thought for the total electromagnetic energy he found was proportional to [Bucherer 04]:

$$\frac{1}{2}\ln\left(\frac{1+\beta}{1-\beta}\right)-\beta.$$

This expression happens to be the difference between the measures of distance in hyperbolic and Euclidean velocity spaces. When the radius of curvature, $c$, becomes infinite, the total electromagnetic energy will vanish, and we return to Euclidean space. So Abraham's total electromagnetic energy was a measure of the distance into hyperbolic space which depended on the magnitude of the electron's velocity.

Abraham's model fell into disrepute, and even Abraham abandoned it in latter editions of his second volume of *Theorie der Elektrizität*. However his electron turns out to be a cigar-shaped, prolate ellipsoid when in motion, while Lorentz's was a pancake-shaped, oblate ellipsoid. So the two models were complementary to one another; the former belonging to hyperbolic velocity space while the latter to elliptic velocity space, with the transition between the two being made by 'inverting' the semimajor and semiminor axes.

### 1.1.3 *Appropriation of Lorentz's theory of the electron by relativity*

Another historian of science, Russell McCormmach [70], claims that:

> Einstein recognized that not only electromagnetic concepts, but the mass and kinetic energy concepts, too, had to be changed. Entirely in keeping with his goal of finding common concepts for mechanics and electromagnetism, he deduced from the electron theory elements of a revised mechanics. In his 1905 paper he showed that all mass, *charged or otherwise*, varies with motion and satisfies the formulas he derived for the longitudinal and transverse masses of the electron. He also found a new kinetic energy formula applying to electrons and molecules alike. And he argued that no particle, charged or uncharged, can travel at a speed greater than that of light since otherwise its kinetic energy becomes infinite. He first derived these non-Newtonian mechanical conclusions for electrons only. He extended them from electrons to material particles on the grounds that any material particle can be turned into an electron by the addition of charge "*no matter how small*." It is curious to speak of adding an indefinitely small charge, since the charge of an electron is finite. Einstein could speak this way because he was concerned solely with the "*electromagnetic basis* of Lorentzian electrodynamics and optics of moving bodies" [italics added].

The argument that takes us from electrodynamic mass to mass in general is the following. Kaufmann and others have deflected cathode rays by electric and magnetic fields to find the ratio of charge to mass. This ratio was found to change with velocity. If charge is invariant, then it must be the mass in the ratio that increases with the particle's velocity. These measurements cannot be used to confirm that all the mass of the electron is electromagnetic in nature. The reason is that "Einstein's theory of relativity shows that mass as such, *regardless of its origin*, must depend on the velocity in a way described by Lorentz's formula" [italics added] [Born 62].

In a collection dedicated to Einstein, Dirac [86] in 1980 observed

> In one aspect Einstein went much farther than Lorentz, Poincaré and others, namely in assuming that the Lorentz transforms should be applicable in all of physics, and not only in the case of phenomena related to electrodynamics. Any physical force, that may be introduced in the future, must be consistent with Lorentz transforms.

According to J. J. Thomson [28],

> Einstein has shown that to conform with the principles of Relativity mass must vary with velocity according to the law $m_0/\sqrt{(1-v^2/c^2)}$. This is a test imposed by Relativity on any theory of mass. We see that it is satisfied by the conception that the whole of the mass is electrical in origin, and this conception is the only one yet advanced which gives a physical explanation of the dependence of mass on velocity.

So this would necessarily rule out the existence of neutral matter, and, in fact, this is what Einstein [05] says when he remarks that charge "no matter how small" can be added to any ponderable body.

The dependencies of mass upon motion arose from the assumption that bodies underwent contraction in the direction of their motion. This follows directly from the nature of the Lorentz transformation. From the geometry of the body one could determine the energy, $W$, and momentum, $G$, since the two are related by

$$dW = v\,dG,$$

in a single dimension. Then since $G = mv$, the expression for the increment in the energy becomes

$$dW = v^2 dm + mv\,dv.$$

Introducing $dW = c^2 dm$, and integrating lead to

$$m/m_0 = 1/\sqrt{(1-\beta^2)}, \tag{1.1.6}$$

where $m_0$ is a constant of integration, and $\beta = v/c$, the relative velocity.

Expression (1.1.6) was derived by Gilbert N. Lewis in 1908. The same proof was adopted by Philipp Lenard, a staunch anti-relativist, in his *Über Aether und Uräther* who attributes it to Hassenöhrl's [09] derivation of radiation pressure. The only verification of a dependency of mass upon velocity at that time was Kaufmann's experiments on canal rays. Kaufmann was able to measure the ratio $e/m$, and assuming that the charge is constant, all the variation of this ratio must be attributed to the mass.

The mass of the negative particle contains both electromagnetic and non-electromagnetic contributions. However, Lewis contended that whatever its origin is mass remains mass so that "it matters not what the supposed origin of this mass may be. Equation (1.1.6) should therefore be directly applicable to the experiments of Kaufmann." But an accelerating electron radiates, and the radiative force is missing from $dG$. This did not trouble Lewis, and he went on to compare the observed value of the relative velocity with that calculated from (1.1.6). His results are given in the following table.

| $m/m_0$ | $\beta$ (observed) | $\beta$ (calculated) |
|---|---|---|
| 1 | 0 | 0 |
| 1.34 | 0.73 | 0.67 |
| 1.37 | 0.75 | 0.69 |
| 1.42 | 0.78 | 0.71 |
| 1.47 | 0.80 | 0.73 |
| 1.54 | 0.83 | 0.76 |
| 1.65 | 0.86 | 0.80 |
| 1.73 | 0.88 | 0.82 |
| 2.05 | 0.93 | 0.88 |
| 2.14 | 0.95 | 0.89 |
| 2.42 | 0.96 | 0.91 |

Although the calculated and observed values of the relative velocities follow the same monotonic trend, the latter are between 6–8% larger. Lewis believed that this was within the limits of experimental error in Kaufmann's experiments. While Kaufmann claimed a higher degree of accuracy is necessary, Lewis believed that

> notwithstanding the extreme care and delicacy with which the observations are made, it seems almost incredible that measurements of this character, which consisted in the determination of the minute displacement of a somewhat hazy spot on a photographic plate, could have been determined with the precision claimed.

So what is Lewis comparing his results to?

Kaufmann's initial results agreed better with the expression,

$$\frac{m}{m_0} = \frac{3}{4}\frac{1}{\beta^2}\left(\frac{1+\beta^2}{2\beta}\ln\frac{1+\beta}{1-\beta} - 1\right),$$

derived from Abraham's model rather than (1.1.6), which coincides with the Lorentz model, but which has been "derived from strikingly different principles." Why neutral matter should be subject to the deflection by the electromagnetic fields in Kaufmann's set-up is not broached. But, Lewis considers that the mass of a positively charged particle emanating from a radioactive source would be a good test-particle because it consists of mainly 'ponderable' matter with a very small 'electromagnetic' mass.

Lewis believed that his non-Newtonian mechanics revived the particle nature of light. From the fact that the mass, according to (1.1.6), becomes infinite as the velocity approaches that of light, it follows that "*a beam of light has mass, momentum and energy, and is traveling at the velocity of light would have no energy, momentum, or mass if it were at rest....*" This is almost two decades before Lewis [26] was to coin the name 'photon' in a paper entitled "The conservation of photons." The paper was quickly forgotten, but the name stuck.

## 1.2 Physicists versus Mathematicians

In attempting to unravel the priority rights to the unification of light and electricity we can appreciate a remarkable confluence of physicists and mathematicians in one single arena that was never to repeat itself. On the physics side there were André-Marie Ampère, Ludwig

Boltzmann, Rudolf Clausius, Michael Faraday, Hermann von Helmholtz, James Clerk Maxwell, and Wilhelm Weber, while on the mathematics side there were Carl Friedrich Gauss and Bernhard Riemann, and those that should have been there, but were not: János Bolyai and Nicolai Ivanovitch Lobachevsky.

To Ampère credit must go to the fall of the universal validity of Newton's inverse square law as a means by which particles interact with one another at a distance. Today, Ampère is remembered as a unit, rather than as the discoverer of that law, and contemporary treatises on electromagnetism present the alternative formulation of Jean-Baptiste Biot and Félix Savart. Although both laws of force coincide when the circuit is closed, they differ on the values that the force takes between two elements of current when open. That the interaction of persisting direct (galvanic) currents needed an angular-dependent force was loathed and scorned at. Surely, magnetism cannot be the result of the motion of charged particles. Odd as it may seem, like many of the French physics community, Biot rejected Ampère's discovery outright.

Since the angular dependencies vanish when electric currents appear in complete circuits, it seemed as extra baggage to many, including Maxwell, who reasoned in continuous fields which could store energy and media (i.e. the aether) in which waves could propagate in. Yet, it was Ampère's attempt that would initiate a search for a molecular understanding of what electricity is and how it works.

### 1.2.1 *Gauss's lost discoveries*

> *It may take very long before I make public my investigations on this issue; in fact, this may not happen in my lifetime for I fear the 'clamor of the Boeotians.'*
> Gauss in a letter to Bessel in 1829 on his newly discovered geometry.

Gauss's seal was a tree but with only seven fruits; his motto read "few, but ripe." Such was, in effect, an appraisal of Gauss's scientific accomplishments. Gauss had an aversion for debate, and, probably, a psychological problem of being criticized by people inferior to him, like the Boeotians of Greece who were dull and ignorant.

Ampère's discovery would have finished in oblivion had it not caught the eye of Gauss. By 1828 Gauss was resolved to test Ampère's angle law when he came into contact with a young physicist, Wilhelm Weber. With no surprise, Weber was offered a professorship at Göttingen three years later, and an intense collaboration between the two began. According to his 1846 monograph, Weber was out to measure a force of one current on the other. This was something not contemplated by Ampère who was satisfied to making static, or what he called 'equilibrium,' measurements.

When Weber was ready to present his results, he shied away from a discussion of the angular force because he knew it would cause commotion. A letter from Gauss persuaded him otherwise, and insisted that further progress was needed to find a "constructible representation of how the propagation of the electrodynamic interaction occurs."

Weber accepted Fechner's model in which opposite charges are moving in opposite directions, and interpreted Ampère's angular force in terms of the force arising from relative motion, depending not only on their relative velocities but also on their accelerations. In so doing, Weber can thus be considered to be the first relativist! The anomaly in Ampère's law, where there appears a diminution of the force at a certain angle, now appeared as a diminution of the force at a certain speed. That constant later became known as Weber's constant, and in a series of experiments carried out with Rudolf Kohlrasch it was found to be the speed of light, increased by a factor of the square root of 2. Present at these experiments was Riemann, and Riemann was later to present his own ideas on the matter.

In the 1858 paper, "A contribution to electrodynamics," that was read but *not* published until after Riemann's death, Riemann states

> I have found that the electrodynamic actions of galvanic currents may be explained by assuming that the action of one electrical mass on the rest is not instantaneous, but is propagated to them with a constant velocity which, within the limits of observation, is equal to that of light.

Although he errs referring to $\phi = -4\pi\rho$ as Poisson's law, instead of $\nabla^2\phi = -4\pi\rho$, Riemann surely did not merit the wrath that Clausius bestowed upon him. Riemann proposes a law of force similar to that of Weber, where the accelerations along the radial coordinate connecting the two particles are replaced by the accelerations projected onto the coordinate axes, and advocates the use of retarded potentials instead of a scalar potential.

In his *Treatise*, Maxwell cites Clausius's criticisms as proof of the unsoundness of Riemann's paper. Surely, Maxwell had no need of Clausius's help, so it was probably used to avoid direct criticism. Moreover, Clausius's criticisms are completely unfounded, and what Maxwell found wanting in Weber's electrokinetic potential actually applies to Clausius's expression. Whereas Clausius had some grounds for his priority dispute with Kelvin when it came to the second law, here he has none.

Weber's formulation,which today is all but forgotten, held sway in Germany until Heinrich Hertz [93], Helmholtz's former assistant, verified experimentally the propagation of electromagnetic waves and showed that they had all the characteristics of light. Helmholtz then crowned Maxwell's theory, and went even a step further by generalizing it to include longitudinal waves, if ever there would be a need of them [cf. Sec. 11.5.5].

Gauss played a fundamental role in bridging the transition from Ampère to Weber. Moreover, Maxwell's formulation of a wave equation, from his circuit equations, in which electromagnetic disturbances propagate at the speed of light, was undoubtedly what Gauss thought was as an oversimplification of the problem. The complexity of the interactions in Ampère's hypothesis persuaded him that it was not as simple as writing down a wave equation for a wave propagating at the speed of light. This will not be the only time Gauss loses out on a fundamental discovery.

Gauss's letters are more telling than his publications, and if it had not been for his reluctance to publish he would have certainly been the discoverer of what we now know as hyperbolic geometry. Gauss wrote another famous letter, this time to Taurinus in 1824, again reluctant to publish his findings. This is what he said:

> ... that the sum of the angles cannot be less than 180°; this is the critical point, the reef on which all the wrecks occur... I have pondered it for over thirty years, and I do not believe that anyone can have given it more thought... than I, though I have never published anything on it. The assumption that the sum of three angles is less than 180° leads to a curious geometry, quite different from ours (the Euclidean), but thoroughly consistent...

Gauss is, in fact, referring to hyperbolic geometry, and it is another of his lost discoveries. The credit went instead to Bolyai junior and Lobachevsky. In 1831, Gauss was moved to publish his findings, as it appears in a letter to Schumacher:

> I have begun to write down during the last few weeks some of my own meditations, a part of which I have never previously put in writing, so that already I have had to think it all through anew three or four times. But I wished this not to perish with me.

But it was too late, before Gauss could finish his paper, a copy of Bolyai's Appendix arrived.

Gauss's reply to Wolfgang Bolyai senior unveils his disappointment:

> If I commenced by saying that I am unable to praise this work, you would certainly be surprised for a moment. But I cannot say otherwise. To praise it, would be to praise myself. Indeed the whole contents of the work, the path taken by your son, the results to which he is led, coincide almost entirely with my meditations, which have occupied my mind partly for the last thirty or thirty-five years. So I remained quite stupefied... it was my idea to write down all this later so that at least it should not perish with me. It is therefore a pleasant surprise for me that I am spared the trouble, and I am very glad that it is just the son of my old friend, who takes precedence of me in such a remarkable manner.

Even more mysterious is why Gauss failed to help the younger Bolyai gain recognition for his work. Was it out of jealousy or Gauss's extreme prudence?

Another person who was looking to the stars for confirmation that two intersecting lines can be parallel to another line was Lobachevsky. He, like Gauss, considered geometry on the same status of electrodynamics, that is, a science founded on experimental fact. Lobachevsky fully realized that deviations from Euclidean geometry would be exceedingly small, and, therefore, would need astronomical observations. Just as Gauss attempted to measure the angles of a triangle formed by three mountain-tops, Lobachevsky claimed that astronomical distances would be necessary to show that the sum of the angles of a triangle was less than two right angles.

In 1831 Gauss deduced from the axiom that two lines through a given point can be parallel to a third line that the circumference of a circle is $2\pi R \sinh r/R$, where $R$ is an absolute constant. By simply replacing $R$ by $\mathrm{i}R$, he obtained $2\pi R \sin r/R$, or the circumference of a circle of radius $r$ on the sphere. The former will be crucial to the geometrical interpretation of the uniformly rotating disc that had occupied so much of Einstein's thoughts. And we will see in Sec. 9.11 that Gauss's expression for the hyperbolic circumference is what modern cosmologists confuse with the expansion factor of the universe.

The first person to show that there was a complete correspondence between circular and hyperbolic functions was Taurinus in 1826, who was in Gauss's small list of correspondents on geometrical matters. Although this lent credibility to hyperbolic geometry, neither Taurinus nor Gauss felt confident hyperbolic geometry was self-consistent. In 1827 Gauss came within a hair's breadth of what would latter be known as the Gauss–Bonnet theorem. This theorem shows that the surfaces of negative curvature produce a geometry in which the angular defect is proportional to the area. Gauss was cognizant that a pseudosphere was such a surface, and Gauss's student Minding latter showed that hyperbolic formulas for triangles are valid on the pseudosphere. But, a pseudosphere is not a plane, like the Euclidean plane, because it is infinite only in one direction. The extension of the pseudosphere to a real hyperbolic plane came much later with Eugenio Beltrami's exposition in 1868. So it was not clear to Gauss and his associates what this new geometry was, and, if, in fact, it was logically consistent.

Gauss dabbled in many areas of physics and mathematics, and it would appear that his interests in electricity and non-Euclidean geometries are entirely disjoint. Who would have thought that these two lost discoveries might be connected in some way? Surely Poincaré did not and it is even more incredible because he developed two models of hyperbolic geometry that would have made the handwriting on the wall unmistakable to read.

### 1.2.2 *Poincaré's missed opportunities*

Jules-Henri Poincaré began his career as a mathematician, and, undoubtedly, became interested in physics because of the courses he gave at the Sorbonne. Poincaré was not a geometer by trade, but made a miraculous discovery that the Bolyai–Lobachevsky geometry which the geometers, Beltrami and Klein, were trying to construct already existed in mainstream mathematicians [Stillwell 96]. The tragedy is that he failed to see what he called a Fuchsian group was the same type of transform that Lorentz was using in relativity, and that he would be commenting on the latter without any recognition of the former.

#### 1.2.2.1 *From Fuchsian groups to Lorentz transforms*

Poincaré's first encounter with hyperbolic geometry came when he was trying to understand the periodicity occurring in solutions to particular differential equations. The single periodicities of trigonometric functions were well-known, and so too the double periodicities of elliptic functions. Double periodicity can be best characterized by tessellations consisting of parallelograms in the complex Euclidean plane whose vertices are multiples of the doubly periodic points.

Poincaré found a new type of periodic function, which he called 'Fuchsian,' after the mathematician Lazarus Immanuel Fuchs who first discovered them.[d] The periodic function is invariant under a group of substitutions of the form

$$z \mapsto \frac{az+b}{cz+d}, \tag{1.2.1}$$

for which $ad - bc \neq 0$, for otherwise it would result in a lack-luster constant mapping. Poincaré wanted to study this group of transformations by the same type of tessellations that elliptic functions could be characterized in the complex Euclidean plane. Only now the tessellation consists of curvilinear triangles in a disc, shown in Fig. 1.1, which Poincaré obtained from earlier work by Schwarz in 1872. The curvilinear triangles form right-angled pentagons which are mapped onto themselves by the linear fractional transformation, (1.2.1). As Poincaré tells us

> Just at the time I left Caen, where I was living, to go on a geological excursion ... we entered an omnibus to go some place or other. At the moment I put my foot on the step the idea came to me, without anything in my former thoughts seeming to have paved the way for it, that the transformation I had used to define Fuchsian functions were identical with those of non-Euclidean geometry.

*The linear fractional transformations,* (1.2.1)*, can be used to define a new concept of length for which the cells of the tessellation are all of equal size*. The resulting geometry is precisely that of Bolyai–Lobachevsky which, through Klein's renaming in 1871, has come to be known as hyperbolic geometry.

If $c = b$ and $d = a$, then the fractional linear transformation (1.2.1) becomes the distance-preserving and orientation-preserving map, with $a^2 - b^2 = 1$, of Poincaré's conformal disc model of the hyperbolic plane $\mathbb{D}^2$-isometrics. What Poincaré failed to realize is that by interpreting $z$ as the linear fractional transformation (1.2.1), with $a = \cosh\Psi$ and $b = \sinh\Psi$, becomes precisely the transformation he named in honor of Lorentz, where

[d] After Klein informed Poincaré in May 1880 that there were groups of linear fractional transformations, other than those of Fuchs, Poincaré named them 'groupes kleinéens,' to the chagrin of Klein.

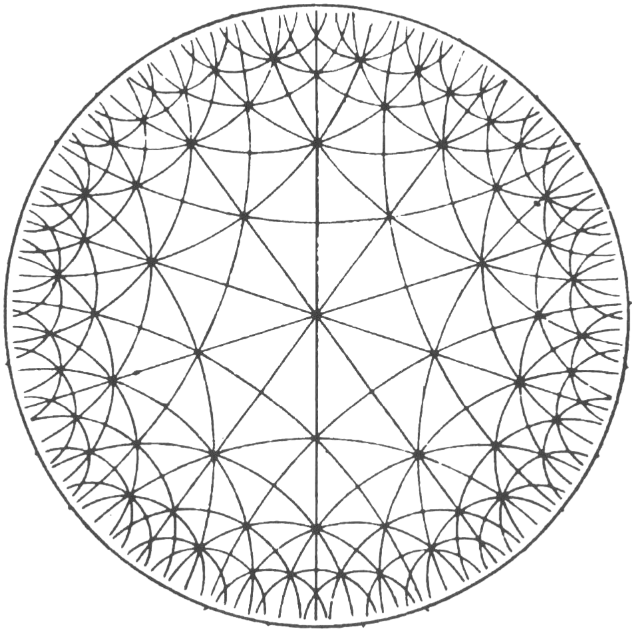

**Fig. 1.1.** A tiling of the hyperbolic plane by curvilinear triangles that form right-angled pentagons.

the sides of any curvilinear triangle in Fig. 1.1 are proportional to the hyperbolic measures of the three velocities in three different reference frames. Had Poincaré recognized this, it would have changed his mind about the 'convenience' of Euclidean geometry, and would have brought hyperbolic geometry into mainstream relativity.

That is, given three bodies moving with velocities $\mathbf{u}_1$, $\mathbf{u}_2$ and $\mathbf{u}_3$, the corresponding triangle with curvilinear sides has as its vertices the points $\mathbf{u}_1$, $\mathbf{u}_2$ and $\mathbf{u}_3$. The relative velocities will correspond to the sides of the triangle and the angles between the velocities will add up to something less than two right angles. It should also be appreciated that the square of the relative velocity is invariant under (1.2.1). Suppose that $\mathbf{w}$ is a relative velocity formed from the composition of $\mathbf{u}$ and $\mathbf{v}$, then if these velocities are replaced by the velocities $\mathbf{u}'$ and $\mathbf{v}'$ relative to some other frame, the value of $\mathbf{w}$ will be unaffected by the change. In other words, the square of the relative velocity $\mathbf{w}$ is invariant under a Lorentz transformation.

However, it never dawned on Poincaré that these curvilinear-shaped triangles might be relativistic velocity triangles for he kept mathematics and physics well separated in his mind. For he considered

> ... the axioms of geometry ... are only definitions in disguise. What then are we to think of the question: Is Euclidean geometry true? We might as well ask if the metric system is true and if the old weights and measures are false; if Cartesian coordinates are true and polar coordinates false. One geometry cannot be more true than another: it can only be more convenient.

Convenience was certainly not the answer.[e]

### 1.2.2.2 *An author of* $E = mc^2$

Unquestionably the most famous formula in all of physics, its origins lie elsewhere than in Einstein's [05b] paper "Does the inertia of a body depend upon its energy content?" John Henry Poynting [07] derived a relation between energy and mass from the radiation pressure around the turn of the twentieth century. Friedrich Hassenöhrl [04] obtained the effective mass of blackbody radiation as $\frac{4}{3}\varepsilon/c^2$, where $\varepsilon = h\nu$. The same factor of $\frac{4}{3}$ was found by Comstock [08] from his electromagnetic analysis, and represents the sum of the energy and the work done by compression, the latter being equal to one-third of the energy in the ultrarelativistic limit. The sum of the two quantities is the enthalpy, as was first clearly stated by Planck [07], so in Einstein's title 'heat content,' or enthalpy, should replace 'energy content.'

Once again we find evidence of Poincaré's priority in the derivation of the famous formula, and, as we have mentioned, Einstein's recognition of it [cf. p. 23]. In the second edition of his text, *Électricité et Optique*, Poincaré [01] treats the problem of the recoil due to a body's radiation. He considers the emission of radiation in a single direction, and in order to maintain fixed the center of gravity, the body recoils like an 'artillery cannon' (pièce d'artillerie). According to the theory of Lorentz, the amount of the recoil will not be negligible. Suppose, says Poincaré, that the artillery piece has a mass of 1 kg, and the radiation that is sent in one direction at the velocity

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[e]Strangely, we find Einstein [22b] uttering the same words: "For if contradictions between theory and experience manifest themselves, we should rather decide to change physical laws than to change axiomatic Euclidean geometry."

of light has an energy of three million Joules. Then, according to Poincaré, it will recoil a distance of 1 cm.

Actually, the relation between 'electromagnetic momentum' and Poynting's vector appears in a 1895 paper by Lorentz, which was commented and elaborated upon by Poincaré [00] in 1900. He derives the expression between the momentum density, **G**, and the energy flux, **S**, as

$$\mathbf{G} = \mathbf{S}/c^2. \tag{1.2.2}$$

Even earlier in 1893, J. J. Thomson refers to 'the momentum' arising from the motion of his Faraday tubes. It is only later that Abraham [03] introduced the term 'electromagnetic momentum.' Pauli [58] unjustly attributes (1.2.2) to Planck [07] as a theorem regarding the equivalence between momentum density and the energy flux density. According to Pauli,

> This theorem can be considered as an extended version of the principle of the equivalence of mass and energy. Whereas the principle only refers to the *total* energy, the theorem has also something to say on the *localization* of momentum and energy.

Since the magnitude of the energy flux, $S = Ec$, (1.2.2) becomes:

$$mv = E/c.$$

Then introducing $m = 10^3$ grams, $E = 3\times10^{13}$ ergs, and $c = 3\times10^{10}$ cm/sec, Poincaré finds $v = 1$ cm/sec for the recoil speed. Thus, Poincaré derived $E = Gc$, and if $G$ is the momentum of radiation, $G = mc$, so that $m = E/c^2$ is the mass equivalent to the energy of radiation.

Poincaré was infatuated with the break-down of Newton's third law, the equality between action and reaction, in his new mechanics. In a follow-up paper entitled, "The theory of Lorentz and the principle of reaction," Poincaré [00] considers electromagnetic energy as a 'fictitious fluid' (fluide fictif) with a mass $E/c^2$. The corresponding momentum is the mass of this fluid times $c$. Since the mass of this fictitious fluid was 'destructible' for it could reappear in other guises, it prevented him from identifying the fictitious fluid with a real fluid. What Poincaré could not rationalize became 'fictitious' to him.

The lack of conservation of the fictitious mass prevented Poincaré from identifying it with real mass, which had to be conserved under all circumstances. What is conserved, however, is the inertia associated with the radiation that has produced the recoil of the artillery cannon. It is the difference between the initial mass and what is radiated that is equal to the change in energy of the system. Ives [52] showed that

$$m - m' = \Delta m = E/c^2, \tag{1.2.3}$$

where $m'$ is the change in mass after radiation, and $E/c^2$ is the mass of the radiant energy, which follows directly from Poincaré's 1904 relativity principle.

The difference between the Doppler shift in the frequency due to a source moving toward and away from a fixed observer is:

$$\Delta \nu = \frac{1}{2}\nu\left[\left(\frac{1+\beta}{1-\beta}\right)^{1/2} - \left(\frac{1-\beta}{1+\beta}\right)^{1/2}\right] = \frac{\nu v}{c\sqrt{(1-\beta^2)}}. \tag{1.2.4}$$

The frequency shift becomes a nonlinear function of the velocity, just like the expression for the relativistic momentum. But here there is no mass present!

The relation between frequency and energy was known at the time; it is given by Planck's law, $E = h\nu$, so that (1.2.4) could be written as

$$h\Delta\nu/c = \frac{Ev}{c^2\sqrt{(1-\beta^2)}} = G,$$

where $G$ is momentum imparted to the artillery piece due to recoil. It is given by

$$G = \frac{\Delta m}{\sqrt{(1-\beta^2)}}v,$$

if (1.2.3) holds. The derivation is thus split into two parts: A nonrelativistic relation between mass and energy, (1.2.3), which depends only on the central frequency, $\nu$, and a relativistic part that relates the size of the shift to the velocity, according to (1.2.4). It is through the difference in the Doppler shifts that the momentum acquires nonlinear dependency upon the velocity,

$$\frac{1}{2}(e^{\bar{v}/c} - e^{-\bar{v}/c}) = \sinh(\bar{v}/c) = \frac{\beta}{\sqrt{(1-\beta^2)}}, \tag{1.2.5}$$

where $\bar{v}$ is the hyperbolic measure of the velocity whose Euclidean measure is $v$. Equation (1.2.5) also indicates that $c$ is the absolute constant of velocity space. If we multiply (1.2.5) through by $\pi c$, it becomes Gauss's expression for the semi-perimeter of a non-Euclidean circle of radius $\bar{v}$, and absolute constant $c$, that he wrote in a letter to Schumacher in 1831 [cf. Eq. (9.11.24)].

Where is the mass dependence on velocity?

The Doppler shifts refer to a shift in frequency, the frequency is related to an energy, the energy is related to mass; that is, the mass equivalent of radiation. In fact, the *attributed* nonlinear dependence of mass on its speed, (1.2.4), can be obtained without mentioning mass at all!

Poincaré was ever so close to developing a true theory of relativity, but ultimately could not break loose of the classical bonds which held him. It is even a greater tragedy that he could not bridge the gap between his mathematical studies on non-Euclidean geometries and relativity that could have unified his lifelong achievements.

## 1.3 Exclusion of Non-Euclidean Geometries from Relativity

Neither Whittaker, nor Pais, gave any reference to the potential role that non-Euclidean geometries could have played in relativity. Pais pays little tribute to Hermann Minkowski other than saying that Einstein had a change in heart; rather than considering the transcription of his theory into tensorial form as 'superfluous learnedness' (überflüssige Gelehrsamkeit), he later claimed it was essential in order to bridge the gap from his special to general theories.

Minkowski, in his November 1907 address to the Göttingen Mathematical Society, began with the words "The world in space and time is, in a certain sense, a four-dimensional non-Euclidean manifold" [cf. p. 37]. The invariance of the hyperboloid of space-time from the Lorentz transform was identified as a pseudosphere of imaginary radius, or a surface of negative, constant curvature. It is plain from Whittaker's formulas that the Lorentz transformation consists of a rotation through an imaginary angle.

Poincaré too viewed the Lorentz transformation as a rotation in four-dimensional space-time about an imaginary angle and that the ratio of the space to time transformations gave the relativistic law of velocity addition.

But, he could not bring himself to identify the velocity as a line element in Lobachevsky space.

Edwin Wilson, who was J. Willard Gibbs's last doctoral student, and Lewis [12] felt the need to introduce a non-Euclidean geometry for rotations, but not for translations. They assumed, however, that Euclid's fifth postulate (the parallel postulate) held, and, therefore, excluded hyperbolic geometry from the outset, even though their space-time rotations are through an imaginary angle. Had they realized that their non-Euclidean geometry was hyperbolic they would have retracted the statement that "Through any point on a given line one and only one parallel (non-intersecting) line can be drawn."

It would have also saved them the trouble of inventing a new geometry for the space-time manifold of relativity. They do, in fact, disagree with Poincaré that

> it is, however, inconsistent with the philosophic spirit of our time to draw a sharp distinction between that which is real and that which is convenient, and it would be dogmatic to assert that no discoveries of physics might render so convenient as to be almost imperative the modification or extension of our present system of geometry.

Neither their plea nor paper had a sequel.

In the last of his eight lectures, delivered at Columbia University in 1909, we listened to Planck's animosity toward non-Euclidean geometries. Although blown up, and completely out of proportion, Planck was making a statement that he does not want any infringement on the special theory of relativity by mathematicians. Where would this infringement come from? From nowhere else than the Göttingen school of mathematicians, notably Felix Klein.

The Hungarian Academy of Science established the Bolyai Prize in mathematics in 1905. The commission was made up of two Hungarians and two foreigners, Gaston Darboux and Klein. The contenders were none other than Poincaré and Hilbert. Although the prize went to Poincaré, his old friend Klein refused to present him with it citing ill health. According to Leveugle [04] it would have meant that Klein had to publicly admit Poincaré's priority over Einstein to the principle of relativity, and the group of transformations that has become known as the Lorentz group, a name coined by Poincaré in honor of his old friend. This would not have been received well by the Göttingen school for not only did Hilbert come in at second place, it would have been a debacle of all their efforts to retain relativity as a German creation.

Arnold Sommerfeld, a former assistant to Klein, showed in 1909 that the famous addition theorem of velocities, to which Einstein's name was now attached, was identical to the double angle formula for the hyperbolic tangent. The velocity parallelogram closes only at low speeds. This was the first demonstration that hyperbolic geometry definitely had a role in relativity, and its Euclidean limit emerged at low speeds.

Now Sommerfeld would surely have known that the hyperbolic tangent is the straight line segment in Lobachevsky's non-Euclidean geometry. Acknowledgment of his former supervisor's interest in relativity surfaced in the revision of Pauli's [58, Footnote 111] authoritative *Mathematical Encylopedia* article on relativity where he wrote:

> This connection with the Bolyai–Lobachevsky geometry can be briefly described in the following way (this had not been noticed by Varičak): If one interprets $dx^1, dx^2, dx^3, dx^4$ as homogeneous coordinates in a three-dimensional projective space, then the invariance of the equation $(dx^1)^2 + (dx^2)^2 + (dx^3)^2 - (dx^4)^2 = 0$ amounts to introducing a Cayley system of measurement, based on a *real* conic section. The rest follows from the well-known arguments of Klein.

Sommerfeld just could not resist rewriting the history of relativity. He changed Minkowski's opinion of the role Einstein had in formulating the principle of relativity. Quite inappropriately he inserted a phrase praising Einstein for having used the Michelson experiment to show that a state of absolute rest, where the immobile aether would reside, has no effect on physical phenomena [Pyenson 85]. He also exchanged the role of Einstein as the clarifier with that as the originator of the principle of relativity.[f]

A much more earnest attempt to draw hyperbolic geometry into the mainstream of relativity was made by Vladimir Varičak. Varičak says that

[f] And Sommerfeld's revisions did not stop at relativity. Writing in the obituary column of the recently deceased Marion von Smolukowski, Sommerfeld lauds Einstein for his audacious assault on the derivation of the coefficient of diffusion in Brownian motion, "without stopping to bother about the details of the process." Von Laue, writing in his *History of Physics* clearly states that Smolukowski developed a statistical theory of Brownian motion in 1904 "to which Einstein gave definitive form (1905)."

even before he heard Minkowski's 1907 talk, he noticed the profound analogy between hyperbolic geometry and relativity. At low velocities, the laws of mechanics reduce to those of Newton, just as Lobachevskian geometry reduces to that of Euclidean geometry when the radius of curvature becomes very large. To Varičak, the Lorentz contraction appears as a deformation of lengths, just as the line segment of Lobachevskian geometry is bowed.

Taking the line element of the half-plane model of hyperbolic geometry, Varičak says that it cannot be moved around without deformation. Thus, he queries whether the Lorentz contraction can be understood as an anisotropy of the (hyperbolic) space itself. Varičak also appreciates that in relativity the velocity parallelogram does not close; hence, it does not exist, and must be replaced by hyperbolic addition, which is the double angle formula of the hyperbolic tangent. Relativity abandons the absolute, but does introduce an absolute velocity, $c$; this corresponds to the absolute constant in the Lobachevsky velocity space.

Owing to the fact that an inhabitant of the hyperbolic plane would see no distortion to his rulers as he moves about because his rulers would shrink or expand with him, Varičak questions the reality of the Lorentz transform. To Varičak, the "contraction is, so to speak, only a psychological and not a physical fact." Although known non-Euclidean geometries were not entertained by Einstein, Varičak's formulation should have raised eyebrows. But it did not. The only thing that it would do, by questioning the reality of the space contraction, would be to cause confusion, and this provoked a response by Einstein himself. But whose confusion did he abate?

Apart from optical applications referring to the Doppler shift and aberration, which were already contained in Einstein's 1905 paper in a different form, Varičak produced no new physical relations or new insights into old ones. These factors led to the demise of the hyperbolic approach to relativity, as far as physicists were concerned.

However, there was an isolated incident in 1910, where Theodor Kaluza [10] draws an analogy between a uniformly rotating disc and Lobachevskian geometry. Kaluza writes the line element as

$$\int \sqrt{1+\frac{r^2}{1\pm r^2}\left(\frac{d\varphi}{dr}\right)^2}\,dr, \tag{*}$$

which at constant radius becomes

$$\int \frac{r^2}{\sqrt{1 \pm r^2}} d\varphi. \qquad (**)$$

If Kaluza wants to show that the circumference of a hyperbolic circle is greater than its Euclidean counterpart, he has to choose the negative sign in expression (*), bring out the *dr* from under the square root, and remove the square in the numerator of (**). Apart from these typos, and the fact that the first factor in (*) had to be divided by $(1 - r^2)^2$, Kaluza was the first to draw attention to the fact that the hyperbolic metric of constant curvature describes exactly a uniformly rotating disc. The paper was stillborn.

Another unexplainable event is that Einstein entered into a mathematical collaboration with his old friend, Marcel Grossmann, to develop a Riemannian theory of general relativity. Grossmann was an expert in non-Euclidean geometries; so why did he not set Einstein on the track of looking at known non-Euclidean metrics instead of putting him on the track of Riemannian geometry? Probably Einstein wanted the general theory to reduce to Minkowski's metric in the absence of gravity which meant that the components of the metric tensor reduce to constants. But that meant he was fixing the propagation of gravitational interactions at the speed of light. Grossmann is, however, usually remembered for having led Einstein astray in rejecting the Ricci tensor as the gravitational tensor [Norton 84].

In order for it to reproduce correctly the curvature of 'space-time,' the coefficients would have to be (nonlinear) functions of space, and maybe even of time. According to Einstein the Riemannian metric should play the role of the gravitational field. Curvature would be a manifestation of the presence of mass–energy so that if he could find a curvature tensor, comprising of the components of the metric tensor, then by setting it equal to a putative energy–momentum tensor he could find the components of the metric tensor, and thereby determine the line element.

Such an equation would combine time and space with energy and momentum. The rest is history and has been too amply described by historians of science. Since the metric has ten components, the search was on for a curvature tensor with the same number of components. The contraction of the Riemann–Christoffel tensor into the Ricci tensor, having ten components, seemed initially as a good bet to be set equal the energy–momentum tensor. Setting the Ricci tensor equal to zero was made a condition for the emptiness of space. It constitutes Einstein's law of gravitation, and as Dirac [75] tells us

> 'Empty' here means that there is no matter present and no physical fields *except the gravitational field*. [italics added] The gravitational field does not disturb the emptiness. Other fields do.

So gravity can act where matter and radiation are not!

When the field is not empty, setting the Ricci tensor equal to the energy–momentum tensor leads to inconsistencies insofar as energy–momentum is not conserved. If the Ricci tensor vanishes then so do all that is related to it, like the scalar, or total, curvature. Einstein found that if he subtracted one-half the curvature-invariant from the Ricci tensor and set it equal to the energy–momentum tensor, then energy–momentum would be conserved.

The equipment needed to carry out the program involves, curvilinear coordinates, parallel displacement, Christoffel symbols, covariant differentiation, Bianchi relations, the Ricci tensor and its contraction, plus a knowledge of what the energy–momentum tensor is. The only outstanding solution is known as the Schwarzschild metric, in which the metric is constructed on solving the 'outer' and 'inner' solutions [cf. Secs. 9.10.3 and 9.10.4]. All the known tests of general relativity are independent of the time-component of the metric, except for the gravitational shift of spectral lines, which is independent of the spatial component. The latter was predicted by Einstein in 1911, prior to his general theory of relativity. However, it does not follow from the Doppler shift so Einstein was either uncannily lucky, or the true explanation lies elsewhere.

Viewed from a pseudo-Euclidean point of view, there is a clear distinction between special and general relativity. Within the hyperbolic framework, this separation between inertial and noninertial ones becomes blurred. This is because the uniformly rotating disc is, as Stachel [89] claims, the missing link to Einstein's general theory. That the Beltrami metric describes exactly the uniformly rotating disc, means that hyperbolic geometry is also the framework for noninertial systems.

We have already seen Planck's hostility to non-Euclidean geometries. There was also Wilhelm Wien, Planck's assistant editor of the *Annalen*, who insisted that relativity has "no direct point of contact with non-Euclidean geometry," and Arnold Sommerfeld who considered the reinterpretation of relativity in terms of non-Euclidean geometry could "be hardly recommended." Authoritarianism carried the day and non-Euclidean geometry was shelved for good. It is the purpose of this monograph to show that non-Euclidean geometries make inroads into relativistic phenomena and warrant our attention.

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